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published in
Journal of Sport and Exercise Psychology
1998

document version
Publisher's PDF, also known as Version of record

COMMENT

JOURNAL OF SPORT & EXERCISE PSYCHOLOGY, 1998, **20**, 321-327

Creating Your Own Imaginary World: A Reply to Collins and Hale (1997)

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Progress in scientific knowledge is not only achieved through distinct and qualitatively high research but also through vivid and profound discussion. Therefore, we applaud Collins and Hale's (1997) comment on our paper (Bakker, Boschker, & Chung, 1996) that tested predictions derived from Lang's bio-informational theory of emotional imagery in the domain of sport psychology. Though Collins and Hale make some fruitful comments, we believe that most of their remarks are not convincing.

Imagination is omnipresent. People utilize their imagination to achieve a diversity of goals: to relax, to experience excitement, to enjoy their holidays over and over again, to resolve a mathematical problem, to create new objects, to find solutions for persistent practical problems, to acquire stress management skills, and to practice motor tasks, to mention only a few examples. However, notwithstanding this variety, all imagery has one feature in common: it is essentially a cognitive process (cf. Murphy & Jowdy, 1992), and hence images do not have to obey the laws of the physical world. That is one of the reasons why imagination is such an attractive pastime. You can meet beloved people who are thousand miles away, ignore gravity, and fly like a bird or jump over 2.40 meters—just what you like. Your imagery may accurately reflect the real world, but that is not imperative. You are allowed to create your own reality and fill in the world according to your own ideas. Something like this seems to have happened when Collins and Hale (1997) read Bakker, Boschker, and Chung's (1996) paper on changes in muscular activity while imagining weight lifting using stimulus or response propositions. In a way, Collins and Hale (1997) created a world of their own and commented on that. What is disputed by Collins and Hale (1997) and what is the reality of our paper? We will follow Collins and Hale's comments on conceptual issues, methodology, and analysis, in that order.

Conceptual Issues

In our study we tested the hypothesis, derived from Lang's (1977, 1979) theory, that movement imagery will result in greater muscular activity when re-

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sponse propositions are emphasized in the script than when the script emphasizes stimulus propositions (Bakker et al., 1996, pp. 314-315). According to Collins and Hale, we "mistakenly used a perspective based imagery manipulation to test the hypothesis that response propositions are accompanied by more efferent outflow" (p. 207). Collins and Hale state that the instruction "Just feel yourself moving" would be insufficient to represent a response proposition, and that we would have "erroneously equated stimulus propositions with external images and response propositions with internal images" (p. 208).

Let us first notice that in several places in our paper we pointed out that imagery perspectives and the types of propositions distinguished in Lang's theory are not identical (cf. p. 314 and p. 320) as is admitted by Collins and Hale. The central question is whether our instruction to the participants in the study was sufficient to represent a response proposition or not. According to Lang, Kozak, Miller, Levin, and McLean (1980), quoted in our own and in Collins and Hale's paper, response propositions contain assertions about behavior. Part of our instruction ran as follows: "feel yourself . . . holding the dumbbell in your right/left hand. After you have heard the first tone, feel yourself lifting the dumbbell and, at the next tone, feel yourself bringing the dumbbell back to the starting position" (Bakker et al., 1996, p. 317). Note the difference between the actual instruction and the version presented by Collins and Hale ("Just feel yourself moving"). The instruction provided to the participants describes fairly precisely what participants were supposed to do, and what kind of behavioral reactions the imager would have to experience.

Obviously, Collins and Hale are not convinced that this instruction is precise enough. They argue that

> in developing a script for a specific movement, response propositions need to be established by interviewing individuals who have experienced the movement and by drawing on their terminology. If the responses suggested do not "fit" the movements, or if study participants cannot identify with them, the script will have a very limited effect. (p. 208)

We agree that for particular movements it may be necessary to interview participants in order to draw on their terminology. However, we strongly dispute this necessity in our study, in which study participants were asked to imagine a fairly simple movement, one which they had performed several times just prior to imaginatively performing the lifting and delifting. We believe that our participants (university students) were clever enough to understand our instructions.

Collins and Hale continue their criticism by pointing out that external images can contain response propositions, too. We agree, and nowhere in our article is the opposite contended. Just as external images can contain response propositions, internal images can contain stimulus propositions. In fact, if in imagery self-motion is involved, it will be impossible to separate stimulus and response propositions completely. For practical applications, this is not a problem; on the contrary, since in order to achieve maximum vividness of the image, both stimulus and response propositions should be activated. For research purposes, however, the impossibility of separating stimulus and response propositions is a disadvantage. Preferably, one would manipulate both independently. Since this is not possible if movement of a person is involved, the best approach is to emphasize either stimulus or response propositions as much as possible, as we did.

Finally, Collins and Hale (1997) contend that "propositions are confused with the memory trace which they are supposed to elicit" (p. 208). No, they are not. "The image is a functionally organized, finite set of propositions" (Lang, 1977, p. 867). It should be reminded that stimulus and response propositions are properties of the image! Imagery or imagination is the process of activating an image, either spontaneously by the person him/herself, or driven by the instructions of someone else, and involves, by definition, activation of the set (or network) of propositions, the propositions that, according to Collins and Hale, elicit themselves.

Methodology

The first point raised by Collins and Hale (1997) concerns the possibility that residual tension in the muscle, left from the actual dumbbell curls, could have affected the results in our study:

> Therefore, it is quite possible that the purportedly imagery-induced measures included residual tension left from the overt lifts, and that the findings of greater localized activity in the biceps performing the lifts than in the inactive arm may have been due to residual tension. (p. 209)

Yes, we can imagine that this could have happened, and we designed our experiment so that we could examine this possibility. However, this did not happen, as could have been grasped from our results.

Our ANOVA showed a main effect for imagery instructions. Imagery instructions emphasizing response propositions resulted in greater biceps activity ($M = 0.969$ μV) than the imagery condition in which stimulus propositions were emphasized ($M = -0.029$ μV). In addition, the difference between EMG activity in the covertly active arm and that in the passive arm was significant (see Bakker et al., 1996, p. 318, and Figure 1). If these two findings are combined, and it is realized that we did not report a significant interaction between imagery instructions and arm (active/passive), it is evident that Collins and Hale's suggestion of residual tension as an explanation for our findings is very unlikely. If residual tension left from overt lifts had confounded our results, heightened EMG activity in the covertly active arm of study participants who received instructions in which stimulus propositions were emphasized would have shown up. In Figure 1 we present the results of the main effect for imagery instructions, separately for the active and passive arm. Obviously, there is no EMG activity in the active arm of participants who received a stimulus proposition activating script. Residual tension of the overt lifts did not affect our results.

The second point Collins and Hale (1997) make raises the issue of skill: what is the effect of experienced versus inexperienced participants on the data. Collins and Hale write "Bakker et al. include 10 *inexperienced* lifters in their sample of 34 *participants*, yet never bother to analyze this subgroup separately" (p. 209, italics added). In our Method section we wrote: "Twenty-two male and 17 female students [equals 39 *participants*] volunteered to participate in the study. . . . Ten participants (7 males and 3 females) were *experienced* body-builders" (p.316, italics added), thus 29 were inexperienced. Ignoring Collins and Hale's error of the actual numbers of (in)experienced body-builders, their argument makes sense. We did analyze the subgroup of experienced lifters separately (but did not report the results of this analysis). To test EMG differences between experienced and inexperi-

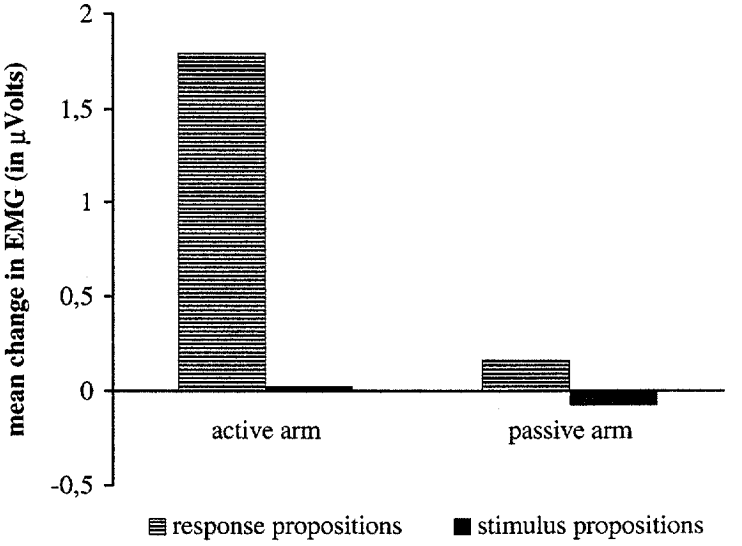

Figure 1 — Mean change in muscular activity of the dominant, "active" arm and the passive arm during the imagined weight lifting when response propositions were emphasized in the imagery script or when stimulus propositions were emphasized.

enced lifters, a 2 × 2 × 2 × 2 (Skill × Imagery Instructions × Arm × Weight) ANOVA, with skill as a between-subject factor and the other factors as within-subject factors, was performed. This analysis of variance revealed no main effect for skill ($F(1, 37) = 0.48$), nor any significant interaction effect between skill and the within-subject factors. These results indicated that for experienced body-builders, changes in EMG activity were not different from those for inexperienced participants.

Collins and Hale raise the issue of (in)experienced body-builders since "the response specificity of the individual's physiological system to particular response propositions is mediated by the individual's past experiences" (Collins & Hale, 1997, p. 209). No doubt that past experience is of relevance. However, once again, Collins and Hale seem to have missed that the task in our study was such that it enabled each subject to perform it easily: it was a simple task. Extensive experience with this task is in no way required for understanding the response propositions entailed in the imagery instructions.

In the same paragraph Collins and Hale point to the lack of support for our third hypothesis, which stated that there will be a relationship between self-report and objective indicators of imagery ability. We were not able to sustain this hypothesis. Contrary to Collins and Hale, we are not worried about the failure to support this hypothesis. Almost 25 years ago, Rehm (1973) concluded in her study on relationships among measures of visual imagery: "This study is basically in agreement with similar studies in finding no behavioral measures of visual imagery which covary with and therefore supplement self-report ratings of imagery vividness" (p. 270). Since then, the general vein of research findings in this area seems to underscore this conclusion (e.g., Finke, 1989), and there are few arguments to expect different outcomes if other modalities than visual imagery are involved. In fact, taking into consideration the complexity of (visual) imagery ability

(cf. Kosslyn, 1994, p. 379-388), it is even very unlikely that a self-report measure of this ability should reach a high level of validity.

We checked on the effects of the different scripts in the different conditions by asking study participants to fill in the Imagery Rating Scale (IRS) immediately following each imagery task. Collins and Hale dispute the suitability of this IRS, because it only asked, on a 7-point Likert-type rating scale, how difficult or easy it was to imagine the lifting movement. This criticism of the IRS is odd since we used exactly the same wording in our instructions to the IRS as is applied in the Movement Imagery Questionnaire (MIQ, Hall & Pongrac, 1983), which we used to measure imagery ability. The MIQ is generally accepted as an instrument that seriously attempts to measure how vivid and controllable an image is. This is also admitted by Collins and Hale; witness their concerns about our third hypothesis, as discussed above.

Analysis

Collins and Hale criticize that we chose to measure simply the amplitude of muscular activity of the agonist and that we did not consider antagonist action or the frequency and duration of the EMG (see p. 210). The main purpose of our experiment was to test predictions derived from Lang's theory. In Lang's (1977) theory, the propositional structure of the imagined scene refers to specific response systems. On the basis of this premise we predicted differences in location and magnitude of the EMG signal during imagery, depending on the instructions and the weight of the dumbbell. We did not predict that the EMG signal during imagery would be like the signal of an actual movement. On the contrary, in our Discussion we referred to Bernstein's (1967) principle of functional nonunivocality, "which implies that there does not exist a one-to-one relationship between neural activation and motor actions," to emphasize that "it is unlikely that the content of an image is reflected in all detail in concomitant EMG activity" (Bakker et al., 1996, p. 321).

Collins and Hale suggest that we should consider the duration of the muscular activity. Lang (1979) stated that during active imagery the pattern of effector activity provides "a real-time index of the image processing going on in the brain" (p. 501). On a general level (e.g., when comparing an active phase with a rest period), a resemblance can be found between the occurrence of muscular activity and changes in the content of the image (see Suinn, 1984). However, considering the principle of nonunivocality, a detailed mirroring of temporal changes in the magnitude of the EMG with the changes in the image itself is not very likely. Although, it would be interesting to examine to what extent such temporal changes in the efferent outflow resemble the changes within the image, such a question was beyond the scope of the hypothesis we tested.

We predicted differences in EMG activity depending on the weight of the dumbbell; lifting a heavy weight requires more strength than lifting a light weight, and we expected that these differences would show up during imagery instructions that activated response propositions. Muscle tension or strength might be assessed by means of the EMG activity, that is, by means of its amplitude (or integrated EMG) or frequency. Amplitude is positively and well correlated with muscle strength, and frequency is also correlated with muscle strength (Stålberg, 1980). Although, during continuous activities (like the numerous dumbbell lifts in our study), the frequency of an EMG signal generally shifts towards lower frequencies

(Stålberg, 1980). For the task we used, such a possible change in the frequency of the EMG implies that the frequency might be a less reliable parameter than the amplitude. That is the reason why we decided to operationalize EMG activity as the amplitude of the EMG signal (Bakker et al., 1996, p. 318). This does not rule out the possibility that for another task or type of experimental design the frequency might be suitable as well.

Although, it might be interesting to examine the EMG activity of both agonist and antagonist, as Collins and Hale suggest, it should be noticed that during a dumbbell lift (as for most limb movements) agonist as well as antagonist muscles will be active. This consideration led us to the decision to measure only EMG activity of the agonist (see p. 318).

Collins and Hale (1997) contend that "Ideally, future research will attempt to measure the frequency, duration, and amplitude of imagery concomitants through a variety of psychophysiological techniques (e.g., EMG, EEG) and measures of cerebral blood flow or CAT scan technology (Decety & Ingvar, 1990), while simultaneously examining the effects of image efference on motor learning and performance" (p. 211). We do not believe that this type of statement is very valuable for outlining future research directions. Recently, Decety et al. (1994) studied brain activity during passive observation of movements while imagining performing these movements. Decety et al. were not interested in EMG concomitants of imagery; thus they used EMG activity only to check for the absence of actual movement during imagery. Similarly, Decety et al. did not examine the effects of image efference on motor learning and performance. Nevertheless, their results seem to permit conclusions about the role of imagery in motor learning and their study is quite relevant for the area of imagery in sports. Thus, ideally, future research will measure the variables that are relevant for the hypotheses tested in the study.

We will end with a more general comment on Collins and Hale's criticism. Under every subheading of their paper, Collins and Hale deliver some general statements that are, in themselves, correct. The last example nicely illustrates this. In general, the combination of, for example, peripheral physiological measures with simultaneous measurement of central brain processes is an interesting approach (e.g., Cunnington, Iansek, Bradshaw, & Phillips, 1996) in the study of imagery. The suggestion of Collins and Hale to focus on these relationships is a valuable one. However, discussing this suggestion under the heading "analytical weakness" when referring to our study is misleading. The suggestion has nothing to do with the study that is criticized. Similar inappropriate statements are included in the other sections of Collins and Hale's (1997) comments on our study (e.g., "Furthermore, visual and kinesthetic aids and exercises [Hale, 1994] to improve vividness and control of imagery may be necessary in addition to image scripts for many athletes" [p. 210]; "For the practitioner, the use of individualized feedback sessions, which solicit and then reinforce self-generated response propositions, needs to be the method of choice" [p. 209]). These contentions are not disputed in Bakker et al.'s (1996) paper, neither directly nor by implication.

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Acknowledgments

We wish to express our gratitude to Raôul Oudejans and Piet van Wieringen for their comments on an earlier version of this reply.

Manuscript submitted: January 23, 1998